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ON EXTRANEOUS MINERAL MATTER CONTAINED IN COMMERCIAL GINGER.

By Alfred H. Allen.

In the course of examination of samples of ginger for the detection of an admixture with the previously extracted article, I have occasionally met with samples containing such a large proportion of mineral matter as to suggest strongly an intentional adulteration with warehouse sweepings. To those familiar with the practice that requires drug-grinders to return the full weight of an article sent to them to grind, the presence of such an impurity will excite but little astonishment, while, of course, there will be no difficulty in finding among the sympathisers with adulteration defenders of such a "trade practice."

I have recently met with two samples of ground ginger to which, in my opinion, exception may fairly be taken on the above ground; and in one instance I felt it my duty to certify to the presence of "not less than 5 per cent. of sand and extraneous mineral matter." The sample in question showed on duplicate analysis the unprecedented proportion of 10·6 per cent. of total ash, of which 2·3 per cent. was actual sand insoluble in acid. On agitating the sample with chloroform nearly 8 per cent. of mineral matter mixed with some organic matter was deposited, and, of course, the mineral matter thus separated cannot be natural to the ash of the ginger. The proportion of ash soluble in water, and the constituents of the ash also pointed to the presence of a notable quantity of extraneous mineral matter besides actual sand.

But the excessive proportion of total ash yielded by the sample is itself sufficient to establish the substantial accuracy of the conclusion at which I arrived, as is evident from a consideration of the following summary of the proportion of ash yielded by 104 specimens of ginger. Of these, 34 are analyses of ginger recorded by other chemists, and are all the published results with which I am acquainted. The figures yielded by Japanese ginger are given in order to complete the data; but this variety is rather a curiosity than an article of ordinary commerce, and ought not to be taken into consideration when interpreting the results.

PROPORTION OF ASH YIELDED BY 104 SAMPLES OF GENUINE AND COMMERCIAL GINGER.

Description of Ginger.	Maximum.	Minimum.	Average.	Number of Samples.	Observer.	Reference.
Ginger Roots ...	3·90	3·29	3·66	7	Allen and Moor.	*Analyst*, xix., 126.
Ginger Roots ...	7·02	3·39	4·72	6	C. Richardson.	*U. S. Bulletin*, No. 13.
Ginger Roots ...	4·10	3·10	3·76	6	Dyer and Gilbard.	*Analyst*, xviii., 197.
Ginger Roots ...	5·00	3·10	4·04	5	T. H. Pearmain.	*Analyst*, xix., 125.
Japan Gingers ...	6·58	3·34	5·02	3	B. Dyer.	*Analyst*, xix., 127.
Ground Gingers ...	8·00	3·40	5·00	7	W. C. Young.	*Analyst*, ix., 214.
Ground Gingers ...	7·94	3·45	5·54	7	C. Richardson.	*U. S. Bulletin*, No. 13.
Commercial Ground Gingers	10·65*	2·48	4·42	63	A. H. Allen.	Samples under Food Act.
General Average ...	10·65*	2·48	4·46	104	Various.	—

From the foregoing table it appears that genuine unground ginger-root yields barely 4 per cent. of ash on the average, and only in a single specimen (recorded by C. Richardson) did the ash rise to 7 per cent. Of the 63 samples of commercial ground ginger purchased by inspectors and submitted to me under the Sale of Food and Drugs Act, only two samples contained more than 8 per cent. of ash, one of these being the sample already referred to, while the other contained upwards of 3 per cent. of sand. But, even including these, the average ash of the 63 samples was only 4·42 per cent., while the general average of all the 104 samples examined was but 4·46 per cent. Of course, the contention that genuine ginger may contain 10 per cent. of mineral matter, natural and extraneous, would, if successful, enable the dishonest trader to make at least 5 per cent. of illicit profit on nearly all ginger which passed through his hands.†

On receiving my certificate on the sample of ginger yielding upwards of 10 per cent. of ash, the West Riding authorities issued a summons, and in due course the case was heard. In support of my certificate I put forward the above statistics.

For the defence, Mr. F. M. Rimmington, Public Analyst for Bradford, stated that he had analysed the defendant's portion of the sample, and recognised it as Barbadoes ginger, which was a low quality. The proportion of total ash, he stated, was not excessive, and the "silex" present was of "underground origin," and natural to the ginger. Mr. Rimmington admitted that he had not looked for any extraneous mineral matter other than sand. At this point the magistrates announced that they had decided to refer the case to Somerset House. The referees in due

* This was the adulterated sample already referred to. The next highest sample on record contained 8·18 per cent. of total ash, which included upwards of 3 per cent. of sand.

† Since writing the above I have met with an analysis by Mr. E. W. T. Jones (ANALYST, xi., 75) of a single specimen of ginger which was found to yield 4·80 per cent. of ash on ignition.

course gave a certificate stating that the sample contained 8·82 of mineral matter, of which 2·11 was sand. They went on to certify that "the total amount of sand and other mineral matter present in the sample is high, and would be regarded as excessive if found in a high-class ginger which had been washed before being ground; but the results obtained are not greater than are sometimes found in low-priced ginger which has been ground as imported. We are of opinion that the sample in question affords no evidence of the presence of sand or extraneous mineral matter other than that present in unwashed ginger."* This certificate was signed by Messrs. R. Bannister and H. J. Helm. The Bench held that if people wanted cheap ginger they must expect to have with it a certain amount of dirt, and dismissed the summons, with costs.

I do not think that the difference in the proportion of ash reported by the referees and that found by me is due to any error of analysis, since other chemists have obtained figures some of which agree with those of Somerset House, and others are more nearly in accordance with those obtained by me. In my opinion, the variation is more probably due to the extraneous mineral matter having gravitated to a particular part of the sample.

Since the occurrence of the foregoing interesting case, Mr. W. F. K. Stock has communicated to me the results yielded by 58 samples of commercial ground ginger analysed in his laboratory. He found

28	samples	to yield	between	3	and	4 per cent. of ash
19	,,	,,		4 ,, 5	,,	,,
4	,,	,,		5 ,, 6	,,	,,
5	,,	,,		6 ,, 7	,,	,,
1	,,	,,		7 ,, 8	,,	,,
0	,,	,,		8 ,, 9	,,	,,
0	,,	,,		9 ,, 10	,,	,,
1	,,	,,		10 ,, 11	,,	,,

Of these 58 samples, 51, or 88 per cent., yielded less than 6 per cent. of ash. The maximum was 10·86 and the minimum 2·90 per cent. These samples were bought at intervals over a wide district, and hence, no doubt, fairly represent the ground ginger supplied to the public. The sample yielding 10·86 per cent. of ash was the only one of the number showing more than 8 per cent., and was certified by Mr. Stock to be adulterated. Mr. C. G. Moor informs me that he found eight specimens of commercial ground ginger analysed by him to yield ash varying from 2·98 to 7·00 per cent., with an average of 4·55 per cent.

Since the foregoing statistics were compiled I have received for analysis, under the Sale of Food and Drugs Act, a sample of ground ginger giving the high proportion of 11·9 per cent. of total ash. The character of the mineral matter in this case is, however, very different from that previously referred to. Instead of containing sand, the sample in question showed on analysis the presence of upwards of 8 per cent. of

* It has been pointed out by a brother analyst that the presence of sulphates and calcium salts in abnormal proportion indicated that the sample in question had been bleached, and hence that it was not unwashed ginger.

calcium sulphate. On treatment with a considerable quantity of cold water, the ginger yielded a solution which gave copious precipitates with barium chloride and ammonium oxalate, and on weighing these precipitates, produced under proper conditions, the sulphate and calcium thus estimated left no room for doubt as to the combination in which they existed. The interesting question next arose as to the form in which the calcium sulphate had been introduced. If it were ascribable to a badly managed bleaching process by calcium sulphite, the presence of well-formed crystals of gypsum was to be expected. If added as plaster of Paris, these crystals would be formed (as was proved by experiment) on treating the sample with water. But on agitating the sample with chloroform a deposit was obtained which, under the microscope, was seen to consist largely of crystalline fragments of gypsum, having exactly the appearance and optical characters of a sample of fibrous gypsum with which the deposit from the sample was compared. Hence there is no doubt that the sample contained an admixture of the ground mineral, and it is impossible to attribute its presence to accident or natural causes. But 8 per cent. of anhydrous calcium sulphate corresponds to 10·1 per cent. of gypsum ($CaSO_4 + 2H_2O$), so that the adulteration was practised to the extent of fully 10 per cent.

TWO DISPUTED CASES OF ADULTERATION.

By E. G. Clayton.

Particulars of the following cases were privately sent to Mr. Hehner a short time ago; but it has occurred to me since that at the present juncture they may be of sufficient interest for publication in The Analyst.

1. In 1892 I received a sample of milk, No. ——, and certified that it contained 10 parts of added water. The analytical data are here stated:

Found.		*Calculated.*	
Specific gravity	1028·9	Fat	3·29
Total solids	11·18	Non-fatty solids	7·89
Fat	3·31		
Non-fatty solids	7·87		
Mineral matter	0·675		
Cream	5 per cent.		

The case was referred to the Somerset House analysts, a copy of whose certificate is appended (I omit the dates and sample-number, but the date of receipt at Somerset House was just thirty days later than the date of receipt of the sample by myself):

"The sample of milk referred to in the annexed letter, and marked ——, was received here on the —th ult. securely sealed. We hereby certify that we have analysed the milk, and declare the results of our analysis to be as follows:

Non-fatty solids	8·08 per cent.
Fat	3·13 ,,
Water	88·79 ,,

"From a consideration of these results, and after making addition for natural loss arising from the change which has occurred in the milk through keeping, we are of opinion that the milk contains not less than 3 per cent. of added water. As witness our hands, this —— day of ——, 1892.

"(Signed) { "J. Bell, D.Sc., F.R.S.
"R. Bannister, F.I.C., F.C.S.
"John Holmes, F.I.C."

At the adjourned hearing in court, in consequence of the fact that the Somerset House certificate (based on an analysis of a sample one month old) appeared to show that the milk was not adulterated to the extent certified to by myself, but only in a slight degree, the case was, on the advice of the magistrate, *withdrawn*.

2. In 1891 I received a sample of coffee, No. ——, and certified that it contained 84 parts of chicory. At the hearing the defendant denied having added more than 50 per cent. of chicory (!). The case was adjourned, and referred to Somerset House. The analysts there certified that the sample "contained not less than 33 per cent. of chicory." The magistrate said that the certificate (*i.e.*, the Somerset House certificate) did not say how much more, and was very indefinite; he then inflicted a fine of £3 and 12s. 6d. costs. (These particulars are taken from the newspaper report.)

Although in this case there was no miscarriage of justice, the vagueness of the wording of the referees' certificate was such as to invoke a magisterial criticism. The disputed milk case calls for no comment, its general similarity to numerous others being sufficiently obvious.

The foregoing are the only two disputed cases of which I have a personal knowledge.

The Occurrence of Chlorine in Saltpetre. A Hellich. (*Chem. Zeit.*, 1894, xviii., 485, 486.)—The author examined a sample of saltpetre (which gave indications when tested directly of being almost free from chloride) for bromate by deflagrating a mixture of the salt with sugar, extracting the residue, acidulating and precipitating with silver nitrate. In spite of the previous proof of the absence of chloride in the original saltpetre, the product of deflagration yielded a precipitate of silver chloride. That this chloride was not derived from any impurity in the sugar was proved by deflagrating the saltpetre with starch, cotton-wool, and sulphur instead of sugar, a precaution which did not prevent the occurrence of a notable reaction for chlorine in the deflagrated mass. When the saltpetre was heated alone, at a temperature not much above its fusing-point for a few minutes, no chlorine reaction was developed; but after stronger and more prolonged heating chlorine as chloride could be detected. When the saltpetre was strongly heated with manganese dioxide the indication of chloride was yet more marked. The examination of various samples of saltpetre was then carried out by the following method, adopted by the author as a standard: 10 grms. of the sample were strongly heated with 0·5 grm. of manganese dioxide until the melt assumed a uniform green colour, when it was dissolved in 50 c.c. of distilled

water, decomposed with 20 c.c. of dilute sulphuric acid, allowed to stand for some minutes, filtered and precipitated with silver nitrate. The purity of the reagents was of course ascertained. That the silver precipitate really consisted of silver chloride was determined by numerous tests.

The author has endeavoured to decide in what form the chlorine is present in the saltpetre, as it does not exist as chloride until the saltpetre has been heated, and he has come to the conclusion that it exists as perchlorate, which, in samples giving the reaction strongly, may exist to the extent of 0·25 per cent. The occurrence of this impurity appears to be common even in "chemically pure" specimens. B. B.

The Analysis of Ethereal Oils. J. Klimont. (*Chem. Zeit.*, 1894, xviii., 641, 642 and 672, 673.)—The most rational system for the examination of ethereal oils is that based on the determination of chemical constants depending on the composition of the oil. At present the observation of a few physical properties is commonly made to suffice for the discrimination of these substances. The determination of the acid value, the saponification equivalent, the methyl number and the carbonyl number (*Sitzungsber. d. K. Ak. Wissensch. Wien. Kl. C. II. Abth. II. b.**), constitutes an important step in this direction, and has now been supplemented by the determination of the terebenthene number, which depends on the fact, observed by the author, that direct quantitative addition of bromine is practicable with essential oils. The process has to be conducted differently from the usual method of determining the bromine absorption, which proved uncertain whether direct titration of the essential oil (in chloroform) with bromine solution, or the plan of adding excess and titrating back, was used. The reagents needed for the execution of the author's method are: (1) a solution of bromine in chloroform of about 1 per cent. strength; (2) an approximately pure terebenthene prepared after Ribau by washing commercial French oil of turpentine with soda, distilling and collecting the fraction passing over between 168° and 170°C.: the substance thus prepared can be kept for months in a well-stoppered bottle provided with a glass cap ground on to the neck; (3) chloroform, not necessarily pure, but identical in quality for any given set of experiments. A pure product may, however, preferably be prepared by shaking with strong sulphuric acid, washing and distilling. The bromine solution is standardized by measuring out about 0·5 c.c. of terebenthene (and determining the exact quantity taken by weight) into a 20 c.c. flask provided with a stopper well ground in. The weighed portion of terebenthene is dissolved in chloroform, filled into a small burette, and used to titrate 10 c.c. of the bromine solution to be standardized. The titration is conducted by adding the terebenthene solution little by little and shaking repeatedly until the bromine solution is completely decolourized. The end-point is sufficiently definite. A corresponding titration with the ethereal oil (in chloroform) to be tested is then made in the same way. As the substances capable of combining with bromine under the conditions of the experiment comprise bodies other than terpenes, the terebenthene number does not necessarily represent the *percentage* of terebenthene in the oil examined, but expresses the total

* No more accessible reference is quoted.—B. B.

bromine absorbed in *terms of terebenthene.* The bromine needs standardizing afresh when more than a day has elapsed since its last standardizing. In order to make the determination of the terebenthene number useful as a means of judging the quality of an essential oil, the author has determined the terebenthene numbers of many samples, the results being given below.

Terebenthene numbers of various oils of turpentine and their adulterants :

Nature of sample.	Terebenthene number.
French oil of turpentine	99·9
American ,, ...	101
Austrian ,, ...	94·9—97·3
Russian ,, ...	70·2—82·5
Hungarian ,, ...	76·8
Russian ,, (deodorised)	80·3—80·9
Rosin oils	52·1—67·6
Petroleum	1·5—1·6

Genuine oils of turpentine (French and American) are distinguished from their adulterants by their high terebenthene number.

Terebenthene numbers of other ethereal oils :

Name of oil.	Terebenthene number.	Colour at end of reaction.
Oil of anise	51·5	
Citronella oil	80·0—82·5	
Oil of lemon	117·7—120·2	
,, cinnamon	22·5—23·8	
,, cassia	21·1—40·9 ...	cherry red
,, rosemary	58·5—59·7	
Angelica oil (seed)	78·4—79·6	
,, ,, (root)	97·4 ...	blue
Oil of peppermint (Japanese) ...	26·2—27·8	
,, ,, (Mitcham) ...	14·5—18·5 ...	rose
,, ,, (red) ...	9·8	
,, caraway	99·2—107·7	
Ol. carvi	77·1—79·5	
Curly mint oil (Krauseminzöl) ...	88·4—89·7	
Oil of fennel	44·9—58·8	
,, orange	119·0—121·1 ...	lemon yellow
Oil of calamus	67·3	
,, bergamot	94·2	
,, cloves	55·1	
,, cajeput...	24·9	
Bajala oil	85·6	
Sandal-wood oil	69·3	
Oil of juniper...	105·2	
Oil of cumin	20·7—24·9 ...	emerald green
Oil of bitter almonds	0·0	
,, rue	10·1	

The author, in commenting upon these figures, points out that the variations in the values obtained for different samples of oil of peppermint are to be ascribed to the state of oxidation (due to keeping) of each sample. The large differences observed in the case of oil of cassia are not readily explicable. At the end of the

paper he insists on the necessity of working a process of this kind, arbitrary as it is, under conditions identical for all samples, and conducting the titration as rapidly as possible, so that any slow secondary absorption of bromine may not be reckoned as that characteristic of the oil under examiation. B. B.

Detection of Perchlorates in the Presence of Chlorides, Chlorates, and Nitrates. F. A. Gooch and D. A. Kreidler. (*Zeits. anorg. Chem.*, 1894, vii., 13; through *Chem. Zeit.*)—The authors have experimented on the detection of perchlorates by evaporating various quantities of a solution of potassium perchlorate to dryness and fusing the residue with anhydrous zinc chloride. Chlorine is evolved by this treatment, and can be recognised by causing it to liberate iodine in the ordinary way. As little as 0·05 milligrammes of potassium perchlorate gave a distinct reaction under these conditions. Chlorides have no influence on the process. Chlorates are previously decomposed by evaporation to dryness with strong hydrochloric acid. In order to prevent nitrates interfering, the dry substance is treated with 2 c.c. of a saturated solution of manganous chloride in strong hydrochloric acid. The liquid is evaporated to dryness, and the residue treated once or twice with 1 to 2 c.c. of strong hydrochloric acid until the nitrate has been completely decomposed. The manganese is then precipitated by the addition of sodium carbonate, and the filtrate is evaporated and heated with zinc chloride, as described above.

B. B.

Comparison of the Kjeldahl-Wilfarth and Stock Methods of determining Nitrogen. E. Cavazzani and A. Cecconi. (*Ann. di Chim. e di Farmakol*, 1894, xx., 87; through *Chem. Zeit.*)—In carrying out Stock's method (THE ANALYST, xvii., p. 109) for determining nitrogen, in which manganese dioxide is added to the sulphuric acid used to attack the substance, the nitrogen of which is to be determined, considerable frothing takes place in the early stages of the process, and care needs to be taken to prevent the liquid being projected from the flask. The end of the reaction can be recognised by the appearance of a green colour in the solution. Comparative analysis by the authors on blood, milk, and urine show that Stock's method is considerably quicker than the Kjeldahl process, but that it yields lower results. The differences are smallest with milk, and more noticeable with blood and urine, amounting to 8—9 per cent. The authors conclude that Stock's process is only available when rapidity of working is more important than extreme accuracy.

B. B.

Colour-reaction for the Detection of Sugars. E. Fischer and W. L. Jennings. (*Ber.* xxvii., 1355-1362.)—Some time ago Professor Emil Fischer showed that when certain sugars were dissolved in an alcohol belonging to the fatty series, and the solution saturated with gaseous HCl, crystalline compounds of the sugar and the alcohol, resembling in some respects the natural glucosides, are formed (see *Ber.* xxvi., 2401). The monohydric phenols appear to be incapable of furnishing such compounds with the sugar, but the polyhydric phenols yield amorphous colourless compounds, which seem also to be analogous to the natural glucosides.

Particularly interesting are the compounds of resorcinol with the aldoses, *i.e.*, sugars containing the group CHO, inasmuch as when these derivatives are oxidized in alkaline solution with such oxidizing agents as lead peroxide, mercuric oxide, and silver oxide, beautiful magenta colorations are produced. One of the most beautiful of these reactions, and one which serves for the detection of all aldoses (or of carbohydrates, which yield aldoses on hydrolysis with acid), is that obtained by warming the new compounds with Fehling's solution. It is so sensitive that in the case of arabinose-resorcinol this compound can be detected in a dilution of 1 : 50,000. The method is carried out as follows: To about 2 c.c. of the dilute aqueous solution of the substance to be tested 0·2 gramme of resorcinol is added, and the mixture saturated with HCl at 0° C. When a large amount of carbohydrate is present the test is allowed to remain at the ordinary temperature for an hour only, but when the quantity of carbohydrate is small it must be put aside for twelve hours. Subsequently the liquid is diluted, an excess of caustic soda added, and then warmed with Fehling's solution (only a few drops of the latter are needful when the quantity of carbohydrate is small). A reddish-violet coloration ensues, which is very characteristic; it is fugitive after a time if the solution is strongly diluted. For the detection of insoluble carbohydrates, such as starch, these are triturated with water, and after adding resorcinol the mixture is saturated with HCl, as already described. The reaction has been proved with saccharose, lactose, maltose, dextrin, gum, glycogen, starch, and cotton-wool, besides the simpler sugars—(pentoses and hexoses). Normal urine exhibits the reaction very markedly.

The test is in many respects similar to that described by Molisch (*Monatsh. Chem.* vii., 198), but is not quite so delicate, and somewhat less convenient; the authors are of opinion, however, that the test may be serviceable in confirming the indications obtained by Molisch's method. A. R. L.

EXTRACTS FROM THE EVIDENCE GIVEN BEFORE THE SELECT COMMITTEE ON FOOD PRODUCTS ADULTERATION, ON JULY 11, 18 AND 25.

MR. RICHARD BANNISTER.

(*Continued from page* 215.)

3. Evidence referring to Milk Standards.

600. You mentioned the standard; have you anything to say about limits?—We went carefully over the whole ground. We have made experiments in the meantime and have always dried to constant weight, and we considered that 8·5 was the point where, if a milk came before us, it was necessary for the vendors to give some explanation about how it was that the milk was so low in solids-not-fat.

601. That is the line at which you begin to be suspicious, as it were, of water, and require a proof to be given that there is no water?—Yes, exactly. There is a form of adulteration of milk that is rather common at the present time, and it is a very dangerous one, that is, the addition of separated milk to new milk. There is no

difficulty there about the solids-not-fat, because the solids-not-fat are increased; you will find that the fat will diminish, but the solids-not-fat will increase. If you find a sample of milk that will contain perhaps from 9 to 9·2 per cent. of solids-not-fat, and fat about 2·7 or 2·8, *it is pretty certain that separated milk has been added to that sample.*

(NOTE.—Refer to Dr. Bell's book for numerous instances of such milks included in his analyses of "genuine" milk.)

602. You think that that is a common method of adulteration now?—I think there is no doubt about it.

1540. What would you make your standard for solids-not-fat and fat?—We make it 8·5 for solids-not-fat and 2·75 for fat.

1728. You have altered your standard recently, have you not?—*We altered the limit from* 2·5 *to* 2·75.

1729. What was your standard before you altered it?—2·5.

1731. And 8·5 of solids-not-fat?—Yes.

1742. Has the Society of Public Analysts the same standard as Somerset House, within your knowledge?—*My opinion,* from what I know from Public Analysts who have consulted us, and from the result of examination, *is that the standard of the Society of Public Analysts seems to be* 8·5 *of solids-not-fat and* 2·5 *of fat.*

1743. Not three?—*Not three.*

1744. I am afraid that the Society of Public Analysts would rather not accept that statement, because, as I understand and am advised, their figures are 8·5 of solids-not-fat and 3 per cent. of fat.—*I have not seen any authority for the* 3 *per cent. of fat.*

1745. You have authority for the 2·5 per cent?—Yes, public analysts at starting made the standard 2·5 of fat.

(NOTE.—See verbatim report of Milk-Committee, ANALYST, vol. x., p. 216.)

1746. Do you not think it would be better if Somerset House and the public analysts could come to some common agreement as to the standard on which they could proceed to make analyses?—Really, so far as public analysts and Somerset House are concerned, the very few variations that there are between Somerset House and the public analysts show conclusively that the method of analysis now pursued is a very good method.

1734. Was it the fear of condemning milk that was pure that has induced Somerset House to keep their standard fairly low? I believe it is somewhat low as compared with what is adopted in other countries, and also by the Society of Public Analysts?—No; I think the whole subject depends upon the Act of Parliament. In other countries you will find that the Act of Parliament is different, and therefore they can go on a different line.

1735. Do I rightly understand you to suggest that the Act of Parliament defines the standard?—No, I do not; but the information that was obtained by the Committee on whose report the Act of Parliament was founded laid it down distinctly that all these little differences that you find in the feeding and keeping of cows, their housing, and different seasons, and all that, had to be taken into consideration.

1736. Then your standard is practically intended to represent a fair average analysis of milks obtained from herds, not from individual cows, because the milk of commerce is admitted, I believe, to be the milk of herds, milk in bulk?—Yes; but the difficulty of interpreting the Act of Parliament in that way arises from the fact that it applies to England, Scotland, and Ireland; and there is no doubt that so far as the large towns are concerned that is the fact; but there is a large quantity of single cow milk distributed in country places, and, therefore, if we had that definition it would follow as a matter of course that a person in a country place might be convicted when he was selling genuine milk.

1737. As a matter of fact, then, you fixed your border-line so as not to exclude single cow milk that might be poor?—But there are a great many single cows that come far below 8·5.

1738. In solids-not-fat?—Yes.

1739. What about the fats, too?—Some of the fats are lower.

1740. Lower than 2·75?—Yes, considerably.

1741. Would you condemn those milks?—No, I should not. If I knew the origin of the milks I should not condemn them; it is simply because we do not know the origin of the milk that we are compelled to put in the limit.

1750. With regard to fixing a standard, you are adverse to a hard and fast standard being fixed?—I am.

1635. I think Dr. Vieth has placed on record his analysis of over 120,000 samples of milk?—But, then, Dr. Vieth's experiments, as far as numbers are concerned, are of little value, because he was analyst to the Aylesbury Dairy Company, and, therefore, the examination of those samples of milk was done really in the examination of samples that were offered from the farmers from the dairies.

1636. Do you suggest that in consequence of his official connection with the Aylesbury Dairy Company those analyses were not an honest record?—No, I do not assume that for a moment; but what I mean to say is, that there is a contract entered into by all those dairy companies as to what quantity of solids-not-fat milk shall contain, and what quantity of fat, and it is the analyst's duty to see that the milk that they get comes up to their standard quality.

1637. But the fact remains that he analysed those 120,000 samples of milk?—*So far as the solids-not-fat and fat were concerned.* Of course, in the analysis of a sample of milk, as a rule, an analyst would go more fully into the analysis of that sample than is necessary for a commercial purpose of that kind.

719. Is sugar of milk ever added as an adulterant, or do you attach any importance to its estimation?—We never do estimate it in the analysis of a sample; it goes in the solids-not-fat.

720. I noticed also that in giving the results of analysis you mentioned the casein and albumen together; why was that?—*The only two points that we give in an ordinary analysis of milk are the solids-not-fat and the fat, and all those come in the solids-not-fat.*

1664. With regard to the standards that you may have at Somerset House, the information as to those has been disclosed, I believe, during the hearing of cases in which Somerset House has given evidence?—Yes, the limits.

1665. That is the way in which that information has become public, as a rule; you told us that you had lately altered your standard of milk?—We have altered the limit of fat from 2·5 to 2·75.

1666. Would that be a matter that the public analysts would be acquainted with, or, rather, informed about?—We stated that distinctly to the different analysts who came up to see us. We have not written officially to the Society of Public Analysts.

4. Evidence referring to Decomposed Milk.

1667. With regard to your system of analysing milk, I understand that you have been in the habit of making allowances for decomposition, time-allowances?—We are compelled to make allowance for change, because in the samples of milk that you are speaking of they are old milks, not new milks.

1668. They would naturally be old milks before they arrive at Somerset House?—They must be.

1669. And in order to allow for the decomposition that has taken place you have a method of calculation?—We see what the change consists of, and work it back into the solids-not-fat.

1670. Would the public analyst know that table of allowances?—*It does not affect them in any way*, because they examine the samples of milk when they are fresh.

1671. Do you think that the table of allowances, the time allowances, that you have set forth for your own use, would give the same analytical results when you were dealing with the sample, as were apparent to the public analysts when they were examining the sample in a fresh condition?—Generally, but in special circumstances they would not; and, therefore, we have abandoned the time allowance, and we try to trace up the change in the composition of the sample, and work it back again into solids-not-fat.

1672. I understand that you have rather altered your time allowances in that direction for the last year or two?—I do not think we have; *practically they are the same*.

1673. When these milks come to you in a stale condition, are you obliged to state whether the article analysed is stale; because under the Act the public analysts have to state in their certificate whether the sample is stale, or has been kept too long?—We examine whether it is fit for analysis, or whether there is any change in the composition of the sample that prevents an analysis being made. That is what I think the analysts have to state.

1674. Do you state that on your certificate?—*We do not state it on our certificate*, but if we come across a sample of milk and we find that it has been *improperly kept*, so that it is mouldy or decayed, we do not examine it. We say that it is not in a fit condition for examination.

1675. In how many cases, can you give us an idea, in recent years have you been unable to examine such a sample?—I should think in five or six cases.

1676. Does milk becoming decomposed alter its component parts, so far as solids are concerned?—Yes, the solids-not-fat are altered; the fat remains practically the same.

1677. And it is by this principle of adjustment that you are able to arrive at the correct analysis?—Yes.

1678. May not some of these milks fail to give the same results?—They will not give the same results. If you take two samples of reference milk the results obtained will not be exactly the same, because they have been kept under different conditions; but we will take out the different substances that have been formed, and work them back again into solids-not-fat, so that if there is a quantity of alcohol formed in the one, and in the other there is none, the alcohol would be worked back again into solids-not-fat.

1679. Are you aware as to the conditions under which the samples have been kept before they were submitted to you?—We can generally tell from the analysis of samples under what conditions they have been kept.

1680. But when fermentation takes place, is there not a loss of substance incurred?—Yes; but there is a great deal of substance required to produce a certain quantity of alcohol, and when you have that quantity of alcohol you put it back again into the term of the substance that has been used up in the production of it.

1681. So that you can account by your system for loss of substance caused by fermentation?—We have found it to be so.

1682. You do not think that any of the conclusions which you arrive at by that process would be such as would cause you to give a decision contrary to that arrived at by public analysts?—Practically, *if the former analyses have been correctly made, they would agree with ours.*

1683. And there is no chance, you think, of any injustice being done to a public analyst by reason of this system which you adopt in dealing with milks that must necessarily be stale, many of which are in a state of fermentation before you examine them?—*I consider that there is no risk of injustice.*

1684. Do you ever test these milks for the presence of preservatives?—Yes.

1685. Would a milk bearing preservatives show a different result from a milk that was not preserved?—Yes, one would decompose more than the other.

1686. And when you detect preservatives, do you adopt a different procedure in coming to your final decision?—No; we take the results of the analysis, the quantity of alcohol formed, of ammonia, of acetic acid, and so on, and turn it back again into solids-not-fat.

1687. I refer to the time allowance?—I stated distinctly just now that we had abandoned the time allowance, because we find that in certain conditions the present system is better.

1688. I did not catch that; how long has that time allowance been abandoned, quite recently?—For the last two or three years, but *practically they are the same.*

1689. What are the same?—The two systems; if you take the time allowance and take our present method of examination; only no doubt our present system is the better system from a chemical point of view, because we take the determination of the different substances in the milk, and the changes that have taken place, and work them back again into solids-not-fat, so that it does not depend upon a time allowance.

1690. But in the communication which was referred to by Sir Charles Cameron, from the public analysts to Somerset House, was there not strong exception taken to that system of time allowance, to the system that you had?—I cannot tell all the details that were touched upon in that statement.

1692. Can you tell us what is the method that you adopt now since you abandoned this time allowance, so that the committee may understand how it is that you proceed in the case of stale samples?—Yes; the two substances that would change by keeping are the sugar and the casein. The sugar might be converted into lactic acid, in which there is no change in the solids-not-fat; or a portion of it might be converted into alcohol, a portion of the alcohol might be converted into acetic acid. As regards the curdy matter or casein, a small quantity of that might be converted into ammonia; accordingly we estimate the amount of ammonia; we estimate the amount of acetic or any other acid that may be present; we estimate the amount of alcohol, and then, taking these different estimations, we turn them back again into the amount of solids-not-fat.

1693. In preparing your certificate when you are called in as a court of appeal, as it were, do you certify whether or not any change has taken place in the nature of the article which might tend to interfere with the analysis?—If we consider that any change has taken place in the nature of the article which interferes with the analysis, we should not examine it.

1694. That would come under those samples which you refuse to analyse?—Yes.

1695. Under the Sale of Food and Drugs Act, are you asked for any opinion or to pronounce a judgment on the samples referred to you?—The reference to us under the Sale of Food and Drugs Act is the charge that is made before the magistrate; we have to examine the sample to see whether that charge has been properly made or not.

1696. Do you give a judgment on the charge, as it were, or do you simply express an opinion on the analysis?—We have to certify that in our opinion the analyses give certain results.

1697. I think you are frequently in the habit of using on your certificate these words as regards milk: "We are unable to affirm that water has been added"?—We have not used that form for years.

1698. I presume that the magistrate on seeing those words would rather feel inclined to think that there was disagreement between the public analyst and Somerset House?—*That really would be a misunderstanding, because the expression "unable to affirm" is only that you cannot tell from an examination of the sample whether water has been added or not.*

1699. On your certificate, do you give such information as will enable a magistrate to know whether the public analyst's certificate and yours are at variance on a matter of opinion or on a matter of fact?—*On a matter of fact.*

(Compare reply to questions 556, 557, and 558, September number of ANALYST, p. 212. Mr. Bannister said: I think that the difference is rather more in *the interpretation of the results* than to actual mistakes made. *We differed in the conclusions.*)

1700. I see that you say that your Department had referred to them since the beginning of the working of the Act 411 samples of milk, and that in 311 of these

cases you confirmed the public analyst, and in 96 you disagreed; 311 and 96 cases total 407; consequently there were four samples of milk in regard to which you neither disagreed with nor confirmed the analyst.—I think there must be some omission there or some error. I think there is nothing either one way or the other to come to the conclusion that we did not either agree or disagree with the analyst. I think there is some error in the figures.

(*To be continued.*)

CORRESPONDENCE.

SOMERSET HOUSE AND PUBLIC ANALYSTS.

[*The following correspondence has been received by the Editorial Committee from Mr. Allen with a request for its publication:*]

67, Surrey Street, Sheffield,
July 21, 1894.

Dear Mr. Bannister,—I was much surprised at the evidence you gave on Wednesday last before the Committee on Food Products Adulteration respecting a sample of malt-vinegar, marked "V.1," recently referred to Somerset House by the Sheffield City Magistrates. I have waited until I had the opportunity of seeing your evidence in print before communicating with you, as I thought I must have misunderstood you.

From your replies on page 93 of the evidence, it would almost seem as if you supposed that I had certified to the sample containing 80 per cent. *of acetic acid from wood.* On reference to the letter sent at your request by the Clerk to the Magistrates, you will find that what I alleged was that the sample contained 80 per cent. of *acetic acid derived from other sources than malt*, and only 20 per cent. derived from malted or unmalted grain. It was quite evident from the analysis that a part at least of this adulteration was not a fermentation-product, and therefore, in the face of the decision of the Birmingham Recorder, it would not have been correct to have called it 80 per cent. of *vinegar* from other sources than malt; but it is quite clear that the term "acetic acid from other sources than malt" includes sugar vinegar, and it has been a matter of great surprise to those concerned in the case that the referees did not report to this effect. Nothing was mentioned about wood-acid in the original certificate, or in the letter you received from the Clerk of the Magistrates, and I still fail to understand why you imported that element into the case.

I note that you say there is no test for wood-acid, and yet you certified to the presence of it, and shall be obliged if you will explain to me how you arrived at that conclusion.

I shall be glad if you will state, for the information of myself and the Health Committee of the City of Sheffield, how much, in your opinion, of malt-vinegar—that is, vinegar derived from malted and unmalted grain—the sample in question contained, as that was clearly the question referred to you. I alleged 20 per cent., and from the figures given by the defendant in the witness-box, the calculated amount closely corresponds with this result.

I do not know what the Inland Revenue officer was told when by your instructions he visited the defendant's works; but I have good reason to believe that the acetic acid employed by the defendant was that of the character commercially known as "1—8 acid," specially sold for adulterating vinegar, and containing a notable quantity of alcohol added for the express purpose of deceiving the referees and public analysts. You appear to have fallen into the trap, and supposed it to be a fermentation-product.

I learn from your evidence that you are always ready to communicate information to

public analysts when requested, and shall therefore be obliged if you will let me have a reply to the foregoing questions at your early convenience.—I remain, yours very truly,

(Signed) ALFRED H. ALLEN.

Richard Bannister, Esq.,
Inland Revenue Laboratory,
Somerset House, London, W.C.

The Government Laboratory, Somerset House, W.C.,
July 27, 1894.

Dear Sir,—I am sorry that, through pressure of work connected with the Committee on Food Products Adulteration, I have been compelled to postpone my reply to your letter till now.

The reply that I gave to Mr. Kearley's question arose from his quotation of the reference letter of Mr. Charles E. Vickers, Clerk to the Sheffield justices, which was that the nature of the alleged adulteration consisted of 20 per cent. of malt-grain vinegar, and 80 per cent. of *diluted* acetic acid not derived from malted or unmalted grain.

In your letter you leave out the word diluted, which qualifying word justified me in concluding that the origin of the 80 per cent. of diluted acetic acid was strong acetic acid, which had been diluted to the required strength.—Believe me, yours very truly,

(Signed) R. BANNISTER.

A. H. Allen, Esq.,
Public Analyst, Sheffield.

67, Surrey Street, Sheffield,
July 28, 1894.

Dear Sir,—I am in correspondence with Mr. Bannister, the Deputy-Principal of the Somerset House Laboratory, respecting the certificate he gave on a sample of vinegar recently referred to him by the Sheffield city magistrates.

Mr. Bannister says he misunderstood the nature of my certificate, being misled by the words of your letter of instruction. May I ask you to oblige me with a copy of your letter to Somerset House in reply to their request for information as to the nature of the alleged adulteration.

Thanking you in anticipation,—I am, dear sir, yours truly,

(Signed) ALFRED H. ALLEN.

Charles E. Vickers, Esq.,
Clerk to the Magistrates, Sheffield.

The Court House, Sheffield,
July 30, 1894.

Dear Sir,

Vinegar.

Your letter of the 28th inst. to hand, and, as therein requested, forward you a copy of my letter to Mr. Bannister respecting the above.—Yours truly,

CHAS. E. VICKERS,
per G. H.

Alfred H. Allen, Esq.,
67, Surrey Street, Sheffield.

The Court House, Sheffield,
June 5, 1894.

Sir,

The Sale of Food and Drugs Act, 1875.

Your letter and enclosure—extract from the Local Government Board's circular of September 30, 1875—received, and in reply beg to say that the nature of the alleged adulteration consisted of 20 per cent. of malt-grain vinegar, and 80 per cent. of diluted acetic acid not derived from malted or unmalted grain.—I am, sir, your obedient servant,

CHAS. E. VICKERS (per G. H.),
Clerk to City Justices.

R. Bannister, Esq.,
Inland Revenue Laboratory, Somerset House, London, W.C.

67, Surrey Street, Sheffield,
August 3, 1894.

DEAR SIR,—I enclose you a copy of the certificate which I gave on the sample of vinegar recently referred to you, which you will see differs materially from the description of the accusation given in the letter of instructions of the Clerk to the Magistrates. I quite agree with you that the word "diluted" bears a different signification from "dilute," and justified your misapprehension of the real nature of the accusation. It is a great pity that Mr. Vickers did not send you an actual copy of my certificate.

I note from your evidence that you are always willing to communicate your limits to Public Analysts who apply for them, and I shall therefore be glad if you will inform me what limits of composition you adopt for malt-vinegar. May I also trouble you to state the limits you adopt for tincture of rhubarb, as this is an article which one of my inspectors not unfrequently sends me.

Thanking you in anticipation for an early reply,—Believe me, yours very truly,

R. BANNISTER, Esq., (Signed) ALFRED H. ALLEN.
Somerset House Laboratory, London, W.C.

ANALYST'S CERTIFICATE.

TO THE HEALTH COMMITTEE OF THE BOROUGH OF SHEFFIELD,—I, the undersigned, Public Analyst for the Borough of Sheffield, do hereby certify that I received on April 2, 1894, from Inspector Harrison a sample of "malt-vinegar" for analysis, and was marked "V.1," and have analysed the same, and declare the result of my analysis to be as follows. I am of opinion that the said sample contained the parts as under :

Malt or Grain Vinegar	20	per cent.
Dilute Acetic Acid not derived from Malted or Unmalted Grain ...	80	„ „
	100	

As witness my hand this 18th day of April, 1894.

(Signed) ALFRED H. ALLEN,
At Sheffield.

Government Laboratory, Somerset House, W.C.
August 4, 1894.

DEAR SIR,—In reply to your letter of yesterday, asking for the limits we adopt for Malt-Vinegar and Tincture of Rhubarb, I beg to inform you that we are unable to adopt a limit in either case, and are compelled to make each sample submitted to us stand on its own merits. The difficulties connected with these examinations are, I am sure, well known to you, and you will fully appreciate the difficulty of speaking more specially on the subject.—Believe me, yours very truly,

ALFRED H. ALLEN, Esq. (Signed) R. BANNISTER.

67, Surrey Street, Sheffield.
August 14, 1894.

DEAR SIR,—I duly received, and am much obliged for, your letter of the 4th inst., and to which I should have replied sooner but for the intervention of the holidays.

I fully appreciate the difficulty of laying down limits for such articles as malt-vinegar and tincture of rhubarb, but unfortunately in practice one is constantly required to meet this difficulty. I not infrequently have to report on samples of tincture of rhubarb, which I do to my own satisfaction on the basis of a series of experiments instituted for the purpose ; but I have no certainty that, in the event of my analysis being disputed, and the sample referred to

Somerset House, your opinion will not be formed on wholly different data. I therefore appeal to you to inform me what data and limits of composition you would rely on supposing that to-morrow a sample of tincture of rhubarb were referred to you under the Act.

Similarly, I request you to inform me what data and limits of composition you would rely on in the case of a sample of malt-vinegar referred to you. I am anxious for this information in order to avoid the annoyance and scandal which result from the disagreement of the Referees with the certificate of a Public Analyst. As an instance in point, I may say that a sample of vinegar, which the manufacturers admitted to me to be largely adulterated with acetic acid, was declared by you to be genuine malt-vinegar.

While urgently requesting a reply on the general questions above submitted, I shall be greatly obliged if you will give me your opinion on the following figures, which I obtained by the analysis of a sample sold as "malt-vinegar." The case is quite distinct from the one above-mentioned, where you passed as genuine one admittedly largely adulterated.

Specific gravity	1·0130
Per 100 parts of vinegar :—	
Acetic acid	4·56
Total extractive matter	1·70
Ash	0·34
Alkalinity of ash in terms of K_2O	0·056
Phosphoric acid (P_2O_5)	0·023
Sulphuric acid (SO_3) combined	0·020
Nitrogen	0·029

Requesting the favour of a reply at your early convenience,—Believe me, yours very truly,

R. BANNISTER, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

Laboratory, Somerset House, London.
August 15, 1894.

DEAR SIR,—With reference to the fixing of Standards for tincture of rhubarb and malt-vinegar, I have nothing to add to the remarks contained in my previous letter. It is practically impossible to lay down general limits for such articles, and I think the experience of the last six months, with reference to the composition of malt-vinegar, must prove how imprudent it has been for analysts to rely on so-called standards, which before the Magistrates have been proved to have no solid foundation.

May I trespass on your kindness for such information about the sample of vinegar you refer to, which was passed by us as genuine malt-vinegar, and which admittedly was largely adulterated, as will enable me to identify it. I shall be much obliged if you will do so.—Yours very truly,

ALFRED H. ALLEN, Esq., (Signed) R. BANNISTER.
Public Analysts' Laboratory,
67, Surrey Street, Sheffield.

67, Surrey Street, Sheffield.
August 16, 1894.

DEAR SIR,—I beg to acknowledge your letter of August 15, stating that it is practically impossible to lay down standards for the composition of tincture of rhubarb and malt-vinegar, and that you consider the experience of the last six months with reference to the composition of malt-vinegar proves how imprudent it has been for analysts to rely on so-called standards. I

regret you do not see your way to give me any assistance even in a specific case like that in which I sent you the figures.

I am sorry I must decline to give you the information you request respecting the sample of adulterated vinegar which you certified to be genuine. The manufacturers consulted me when their customer was summoned, and admitted that there was a very large percentage of added acetic acid present. Under the circumstances I declined to assist them in their defence; but they succeeded in having the case referred to Somerset House, and the Referees reported it to be genuine malt-vinegar, much, of course, to the elation of the manufacturers. You will see that I cannot give you the information which will enable you to identify the vinegar without making known to you the name of the firm who consulted me, and as they sought my advice in confidence, I am bound to preserve their secret.—Yours very truly,

R. Bannister, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

Laboratory, Somerset House, London.
August 17, 1894.

Dear Sir,—I am very much surprised that you decline to give me the information I ask for to identify the sample of vinegar you allege we certified as malt-vinegar, when it contained a "very large percentage of added acetic acid," because I have seen the same statement made in a class paper.

If, therefore, the information could, without any breach of confidence to your client, be supplied to a newspaper, I think that common justice, as well as professional etiquette, demands that we should be told what sample it was to which you refer, to enable us to clear up the charge you bring against us.—Yours very truly,

Alfred H. Allen, Esq., (Signed) R. BANNISTER.
Public Analysts' Laboratory,
67, Surrey Street, Sheffield.

67, Surrey Street, Sheffield.
August 18, 1894.

Dear Sir,—I am duly in receipt of your letter of yesterday referring to the sample certified by you to be pure malt-vinegar, but which I was informed by the manufacturers contained a large percentage of added acetic acid. I am not surprised that you have seen the statement made in a "class paper," and notice that *Food and Sanitation* of to-day contains a reference to the matter. I have made no secret of the fact among my brother Analysts and the Authorities for whom I act, and as the Editor of *Food and Sanitation* is very enterprising, it is not astonishing that the information has reached him; but I did not communicate it, nor have I ever contributed to that journal, except such letters as have appeared over my signature.

I think you will see on reflection that it is a very different thing to make freely known the fact that you missed the adulteration, and to do as you desire, which is practically to inform you of the name of the manufacturer. Everyone has his own standard of ethics, and I have no desire to dictate to you on such a subject; but it is not in accordance with my idea of professional etiquette to make known the name of a client who consulted me confidentially on a matter respecting which he was well aware he was a transgressor.

I may remind you that you have not complied with my request for your opinion respecting

certain data obtained by the analysis of a sample of vinegar submitted to me.—Believe me, yours very truly,

R. Bannister, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

Laboratory, Somerset House, London, W.C.
August 20, 1894.

Dear Sir,—I beg to acknowledge the receipt of your letter of the 18th inst., and am certainly very much surprised to learn therefrom that your idea of professional etiquette permits you to put in circulation a statement respecting the analysis of a sample which we are said to report as "pure malt-vinegar," and when I ask you to supply me with the information necessary to prove the truth or otherwise of the statement, you decline to give it.

Happily such a standard of ethics is rare, even in these days of systematic misrepresentation.

My letter of August 4 gives you the reason why I declined to give an opinion on the data you submitted to me. There are many points to be considered in the analysis of a sample of vinegar, in addition to the figures you forwarded.—Yours very truly,

Alfred H. Allen, Esq., (Signed) R. BANNISTER.
Public Analysts' Laboratory,
67, Surrey Street, Sheffield.

67, Surrey Street, Sheffield.
August 23, 1894.

Dear Sir,—I am duly in receipt of your letter of the 20th inst.

Every official action of the gentlemen acting as Referees under the Sale of Food and Drugs Act is naturally of great interest and importance to Public Analysts, and fair subject for comment. Hence I feel bound, in the cause of truth and the interests of my brother Analysts, to make them acquainted with your failure to report the presence of added acetic acid in the vinegar in question; and the more so in the face of your professed inability to give any indication whatever of the standards or limits you have adopted or would adopt in judging of any similar sample referred to Somerset House.

It is so clearly for the public benefit that Analysts should know that their reports on similarly adulterated samples are likely to be contradicted on appeal, that I am surprised you should feel any dissatisfaction with my taking any steps to attain so desirable an object.

I note your implied doubt of the accuracy of my statement that you reported favourably on a sample sold as malt-vinegar which the manufacturers admitted to contain a large percentage of added acetic acid. I regret this, but it is evident that, if my assurance does not carry conviction, nothing short of an application by you to the manufacturer in question would enable you to "prove the truth of the statement." I have already explained the circumstances under which the information came to me, and can only regret that you should suppose it possible to induce me to commit such a flagrant breach of confidence by making the insinuations contained in your letter.—Yours very truly,

Richard Bannister, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

Laboratory, Somerset House, London, W.C.
August 24, 1894.

DEAR SIR,—Your letter of the 23rd inst. has come duly to hand ; and, in acknowledging it, I must state that I should have been quite content if you had, by inquiry, shown the same anxiety to verify the accuracy of the statement made by your client as you have been in keeping back any information which could lead to the identification of the sample described in one of your letters as genuine malt-vinegar, and in another as pure malt-vinegar, neither of which expressions having, to my knowledge, been used in any of our certificates.

This inquiry you have not made, and you make your case worse by giving, as a reason for circulating the report, my "professed inability to give any indication whatever of the standards or limits you have adopted, or would adopt, in judging of any similar sample referred to Somerset House," when you are quite aware you circulated the tale long before my letter of the 4th inst. was written.

I must refer you again to my letters of the 4th and 15th inst. for the reasons I gave for my inability to define a limit in such cases.

I have never wished, or asked, you to commit any breach of confidence, but I naturally expected that you would, as an act of simple justice, be prepared to substantiate any charge you might make against this department ; but I regret to find that this is not the case.—Yours very truly,

ALFRED H. ALLEN, Esq., (Signed) R. BANNISTER.
67, Surrey Street, Sheffield.

67, Surrey Street, Sheffield.
August 28, 1894.

DEAR SIR,—A sample of ground ginger was recently referred to you by the magistrates sitting at Bingley Petty Sessions. In this case you reported the presence of 8·82 per cent. of mineral matter, of which 2·11 per cent. was sand, and stated that this would be excessive if found in high-class ginger which had been washed before being ground, but the results obtained were not greater than were sometimes found in low-priced ginger which had been ground as imported. Will you kindly inform me of the maximum limit of total ash and sand you would permit in a sample of ground ginger of unknown origin and unknown nature, and in the absence of any information whether it was ground before or after being washed ? I shall be further obliged if you will inform me whether you took any steps in the case in question, and, if so, what, to ascertain whether, as a fact, the ginger named had or had not been washed before being ground.

According to a printed account before me of a case recently heard at Wood Green, in which a sample of milk had been referred to Somerset House for analysis, Mr. Helm and yourself are reported to have certified to the presence of 8·25 of solids-not-fat, and 2·62 of fat, at the same time expressing such an opinion as led the magistrates to dismiss the case. I shall be glad to be informed whether the above report is substantially correct, and, if so, what are the limits you *actually* adopt for fat and solids-not-fat, as it would appear from the report in question that you do not in practice adhere to the statement made by you in evidence before the Select Committee on Food Products Adulteration that your limit for solids-not-fat was 8·5 per cent., and that you had recently raised your former standard of 2·5 of fat to 2·75.

The great importance of the practice of Somerset House must be my apology for troubling you in this matter.—Yours very truly,

R. BANNISTER, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

67, Surrey Street, Sheffield.
August 29, 1894.

DEAR SIR,—I find your letter of August 24 awaiting me after a short absence from home.

I have already explained that I have it *on the authority of the manufacturer himself* that there was a large proportion of added acetic acid present in a sample of vinegar referred to you, and the case was dismissed in consequence of the omission from your certificate of any mention of this added acid, which, therefore, I presume your analysis failed to detect. I do not see how I could possibly have further verified "by inquiry" the accuracy of the statement made by my client as to the composition of the vinegar he himself had manufactured; but I may say that I had *previously detected the fact by analysis* of the vinegar, and it was when the manufacturer was informed of this that he admitted the accuracy of my conclusions.

I understand from your letter that you distinguish, and wish me to draw a distinction, between "genuine malt-vinegar" and "pure malt-vinegar" on the one hand, and "malt-vinegar" without either of these descriptive adjectives on the other. If your contention is that the term "malt-vinegar" simply is legitimately applicable to an article containing a large proportion of added acetic acid, it appears to me a pity that Public Analysts should not be made aware that you hold such a very unusual view of the nature of malt-vinegar.

But if you will have the goodness to peruse my letters again, I think you cannot fail to see that I used the term "genuine" and "pure" in contradistinction to *adulterated*, and did not imply that either of these words was actually used in your certificate on the vinegar in question. But, in order that there may be no misunderstanding, I repeat that a sample sold as "malt-vinegar" was certified by a public analyst to contain a large proportion of acetic acid from another source, which accusation was admitted by the manufacturer to be correct. On reference to Somerset House, you reported in terms which implied that the sample was of unexceptionable character, thereby leading to a dismissal of the summons and a deplorable failure of justice.

If you will kindly refer to my letter of August 18, you will see that I there state that I *had* made no secret among my brother Analysts and the authorities for whom I act of your faulty certificate in the above case, and I undoubtedly mentioned the matter at a date considerably prior to the commencement of this correspondence. But now, in the face of your professed inability to give any indication whatever of the standards or limits you have adopted, or would adopt, in judging of any similar sample submitted to you, I feel bound to make my brother Analysts *in general* acquainted with your failure to report the presence of added acetic acid in the vinegar in question. As pointed out in my last, it is clearly for the public benefit that Analysts should know that their reports on similarly adulterated samples are likely to be contradicted on appeal.

I strongly demur to your statement that I am not prepared to substantiate the charge I have made in this matter against your department. On the contrary, I am quite prepared to repeat on oath the statements contained in this and previous letters to you, and not improbably shall have an opportunity of doing so.

In your letter of August 20, you state that there are many points to be considered in the analysis of a sample of vinegar in addition to the data I gave in my letter of August 14. Will you please tell me what these points are, and I will, to the best of my ability, give you the further information you require in order to express an opinion on the sample? I fully agree with you that it is desirable to base an opinion on the fullest possible analytical and physical data; but I trust you will pardon my saying that I do not think the information obtained by an Inland Revenue officer as the result of his visit to a vinegar works should form the basis of a certificate given by the Somerset House chemists on a sample of vinegar

from the same works submitted to them *for analysis* under the Sale of Food and Drugs Act.—Yours very truly,

R. BANNISTER, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

Laboratory, Somerset House, London, W.C.
August 30, 1894.

DEAR SIR,—I have duly received your letters dated the 28th and 29th inst. As I deny your assertion that a sample of vinegar referred here under the Food and Drugs Act was reported as malt vinegar when it contained added acetic acid, and as you are unable to supply any information which will enable me to identify the sample you refer to, I must decline to continue this correspondence.

I will simply add that at the proper time I shall be happy to show the Select Committee on Food Products Adulteration that in the Woodgreen milk case, to which you refer, our certificate of analysis is quite consistent with the evidence I have already given with regard to the composition of milk.—Yours very truly,

ALFRED H. ALLEN, Esq., (Signed) R. BANNISTER.
Public Analyst's Laboratory,
67, Surrey Street, Sheffield.

67, Surrey Street, Sheffield.
August 31, 1894.

DEAR SIR,—I am duly in receipt of your letter of yesterday.

I am not concerned by your denial and implied disbelief of my statement respecting your erroneous vinegar certificate, as there happens to be a third person whose testimony is available in absolute confirmation of my charge. You will learn more about the matter in due time.

I must remind you that there are several points in my previous letters which you have omitted to notice, and for your convenience will re-state them :

1. Will you kindly inform me what information you require in addition to the data given in my letter of August 14th to enable you to express an opinion on the sample of vinegar therein referred to ?

2. Will you kindly state the maximum limits of total ash and of sand you would permit in a sample of ginger of unknown origin, and in the absence of any information whether it was ground before or after being washed ? Also please inform me whether you took any steps in the case of the sample recently referred to you by the Bingley magistrates to ascertain whether, as a fact, the ginger in question had been washed before being ground.

3. Will you please state what are the limits for fat and solids-not-fat in milk which you adopt in practice ?

In your evidence before the Select Committee on Food Products Adulteration you stated that you are always ready to communicate to a Public Analyst who may apply to you your limits or any other information in your power, and I shall accordingly esteem it a favour if you will let me have a reply on the above points at your earliest convenience.

Thanking you in anticipation, I am, yours very truly,

R. BANNISTER, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

Laboratory, Somerset House, London, W.C.
September 1, 1894.

DEAR SIR,—Your letter of the 31st ult. has been duly received, but for the reasons given in my letter of the 30th I must decline further correspondence.—Yours very truly,

ALFRED H. ALLEN, Esq., (Signed) R. BANNISTER.
67, Surrey Street, Sheffield.

67, Surrey Street, Sheffield.
September 3, 1894.

DEAR SIR,—I am duly in receipt of your note of the 1st inst.

I regret that your failure to induce me—by aspersions, unworthy of you and unmerited by me—to commit a flagrant breach of confidence, is regarded by you as a proper and sufficient reason for refusing to afford me such information as you told the Select Committee on Food Products Adulteration you were ready to give to a Public Analyst who applied for it.—Yours very truly,

R. BANNISTER, Esq., (Signed) ALFRED H. ALLEN.
Government Laboratory,
Somerset House, London, W.C.

To the Editors of THE ANALYST.

2, Fisher Street, Red Lion Square, London, W.C.
September 22, 1894.

DEAR SIRS,—In Mr. Bevan's letter appearing in this month's copy of THE ANALYST, he has unknowingly not stated the exact facts of the case as far as my evidence was concerned. My analysis of the sample in question in duplicate was as follows :

		Repeat.
Total Solids	11·43	11·47
Fat	2·82	2·86
Solids-not-fat	8·61	8·61

The report issued by my firm was "very poor milk, but no proof of adulteration." I supported our certificate at the hearing of the case, to the extent of stating that the milk "might be adulterated," or that it "might be a naturally very poor sample," and that "if the former, it was probably both skimmed and watered." It may also interest your readers to know that apart from the scientific evidence, proof was forthcoming that the milk had not been tampered with.

I must ask you in fairness to myself to publish the above facts, in correction of the mistaken view already put forward.—I am, yours faithfully,

A. J. DE HAILES.

To the Editors of THE ANALYST.

4, New Court, Lincoln's Inn, London, W.C.,
September 27, 1894.

DEAR SIRS,—Will you allow me to point out an error in my letter of August 30, published in the September number ?

The Total Solids should read 11·00, and not 11·10.

I should also like to mention that when Mr. De Hailes analysed the sample it was at least fourteen days old.—Yours faithfully,

EDWARD BEVAN.